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THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

The Official Organ of
**THE INTERNATIONAL COMMISSION ON
ZOOLOGICAL NOMENCLATURE**

Edited by
FRANCIS HEMMING, C.M.G., C.B.E.
Secretary to the International Commission on Zoological Nomenclature

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BULLETIN OF ZOOLOGICAL NOMENCLATURE

Volume 10, Double-Part 10/11 (pp. 299-354)

24th July, 1953

CASE No. 33

ARTICLE 25: QUESTION OF THE STATUS TO BE ACCORDED TO GENERIC NAMES AND TRIVIAL NAMES PUBLISHED AS "NOMINA NUDA" IN SYNONYMIES

(Commission's reference Z.N.(S.)372)

DOCUMENT 33/1

By FRANCIS HEMMING, C.M.G., C.B.E.

(Secretary to the International Commission on Zoological Nomenclature)

THE GENERAL PROBLEM RELATING TO THE STATUS TO BE ACCORDED TO ZOOLOGICAL NAMES PUBLISHED AS "NOMINA NUDA" IN SYNONYMIES

I have already explained in the Report on Case No. 7 how in 1948 I was asked, as Secretary to the International Commission on Zoological Nomenclature, to carry out an investigation, in conjunction with interested specialists, into the question of the species to be accepted as the type species of a nominal genus, the name of which was first published in a generic synonymy. I have explained also how in September, 1950, Dr. Ernst Mayr raised the wider issue of the status of any zoological name, whether a generic name or a trivial name, published without an indication in a synonymy and how I took the view that, as Dr. Mayr's proposal, if approved, would render unnecessary any further consideration of the more limited issue on which in 1948 I had been asked to furnish a Report, the best course would be to group under the present wider issue not only all the documents which had been received in regard to it, but also all the documents which were received in response to the appeal which I made at the beginning of 1952 (*Bull. zool. Nomencl.* 7: 109-118) for assistance and advice from interested specialists on the limited issue which had been referred to me in 1948. I should add that I have included here also statements which I received from certain specialists in response to a short note which I wrote shortly after the receipt of Dr. Mayr's paper. This latter was published in the following serial publications: (1) *Journal of Palaeontology*; (2) the *Geological Magazine*. The reason why only these two publications were asked to publish this note was to ensure that the important issues raised by Dr. Mayr should be brought prominently to the attention of palaeontologists; it was considered that, so far as neontologists were concerned, these would be brought adequately to attention, partly by the wide circulation given to Dr. Mayr's

paper in manuscript and partly by its eventual publication in the *Bulletin of Zoological Nomenclature*. It will be recalled also that in the general appeal for advice and assistance contained in my paper of 1952 (referred to above) attention was drawn not only to the limited issue raised in 1948 but also to the wider problem later raised by Dr. Mayr. Finally, I should note that, prior to the receipt of Dr. Mayr's paper of 1950, representations regarding the Paris interpretation of *Opinion 4* (which forms an important part of the argument advanced by Dr. Mayr) had been received from Mr. J. E. Collin (Newmarket, England) and others. This subject would, therefore, in any case have been placed on the Agenda for discussion at Copenhagen. The papers received in this way are also included in the present Series. The entire series of papers has been arranged chronologically.

2. The fact that the documents now submitted owe their origin to a number of independent sources naturally produces a somewhat ragged appearance and might have rendered difficult the weighing of the views expressed if these had shown any marked differences. It will be seen, however, from the documents now submitted, that there is a remarkable measure of agreement on the main issue involved, though naturally there are some loose ends which it will be necessary to tie up when this matter comes to be discussed at Copenhagen.

3. We may start by noting that, while there is not absolute unanimity of view expressed in the annexed documents, there is an overwhelming majority of opinion in favour of amending the *Règles* in such a way as to deny availability to names—both generic names and trivial names—when published in synonymies, whether generic synonymies or specific synonymies, without any independent diagnosis or other “indication” being given for the taxonomic unit (whether genus or species), the name of which was so synonymised. Further, there is a strongly expressed sentiment in favour of introducing safeguards designed to prevent the upsetting of names published in synonymies in cases where such names are well-established as the result of the application by workers of the principle enunciated in the Commission's *Opinion 78* (1924, *Smithson. misc. Coll.* 73 (No. 2): 1-14), which is inconsistent with the general view now expressed as to the treatment which it is desirable should be accorded to names published in synonymies.

4. In considering proposals for securing the end now sought, it will be necessary to take account of the action taken in Paris in 1948 in relation to the status of manuscript names—or “*nomina nuda*”—published in synonymies. Different aspects of this question were so considered on the following three occasions :—

- (1) Paris Session, 6th Meeting, Conclusion 18 (see 1950, *Bull. zool. Nomencl.* 4: 145-146) :

The decision in *Opinion 4* (as then interpreted) was at this point incorporated into the *Règles* as follows :—

"A manuscript name acquires status in zoological nomenclature only when it is validly published and, on being so published, is published in conditions which satisfy the requirements of the provisos to Article 25, and the status of a manuscript name, so published, is not affected by the question whether the author by whom it is published accepts it as an available name or sinks it as a synonym."

- (2) Paris Session, 12th Meeting, Conclusion 36 (see 1950, *Bull. zool. Nomencl.* 4: 350-351):

It was here ruled that a generic name published in a specific synonymy acquires no status thereby in zoological nomenclature.

- (3) Paris Session, 12th Meeting, Conclusion 37 (see 1950, *Bull. zool. Nomencl.* 4: 351-352):

The subject here dealt with was the status of generic names published in generic synonymies. No explicit ruling was given on the question of the availability of a generic name published in this way, but it was clearly assumed that such a name was available, for it was here that it was decided that an investigation should be undertaken regarding the species to be accepted as the type species of a nominal genus, the name of which was published in this way.

5. Of the foregoing rulings it is the first which is called into question by Dr. Mayr's proposal, for the second is fully in line with that proposal, while (as explained above) no ruling was taken as regards the third, and any amendment now taken in regard to the first would cover also the third. It will probably be agreed that at this stage no practical advantage would be served by further discussion as to the meaning originally attaching to *Opinion* 4, for that *Opinion* has now been revoked (see 1950, *Bull. zool. Nomencl.* 4: 165-166) and the decision in it, as interpreted in 1948, has been incorporated into the *Règles*. The practical issue now to be considered is whether, and, if so, how, the Paris decision is to be amended.

6. Of the main proposals embodied in the documents submitted each has as its object the inclusion in the *Règles* of a provision that no name acquires status in zoological nomenclature by virtue merely of being published in the synonymy of some other name. It is part of these proposals that the *Règles* should make it clear that the foregoing ruling does not apply where a name is published in a synonymy but at the same time is accompanied by a description or other "indication" expressly written for the taxonomic unit to which that name is applied, as contrasted with the taxonomic unit with the name of which it is synonymised.

7. The two proposals referred to above are: (a) the proposals submitted by Dr. Ernst Mayr in Document 33/8; (b) the proposals submitted by Professor Chester Bradley in Document 33/26. In dealing with a complicated issue such

as the present, it is often convenient for purposes of discussion to have before one the draft of a decision in a given sense, for such a draft serves to crystallise discussion and to ensure also that all the relevant facets of the problem are taken into account. With this object in view I prepared early in 1952 a document reviewing the comments so far received in regard to the present case, to which I attached drafts of two possible conclusions, one approving the proposals submitted by Dr. Mayr, the other rejecting them. This document I sent to Dr. Mayr (under cover of a letter dated 25th January, 1952) for comment and observations. Dr. Mayr, in replying (on 11th February, 1952) suggested certain modifications in the text of the document but none as regards the drafts of the suggested conclusions. In view of the large amount of additional support for the general point of view advocated by Dr. Mayr, I later decided that the paper in question needed to be completely recast and it is for this reason that I have prepared the present note. In view of the great weight of support since received in favour of providing that *nomina nuda* published in synonymies shall acquire no status in zoological nomenclature in virtue of being so published, I no longer consider it necessary to consider the form which it would be desirable to give to a decision rejecting the foregoing proposal, but I still think that it would be helpful to examine the form which a favourable decision should take. I accordingly attach to the present note the draft conclusion which I prepared in consultation with Dr. Mayr. The proposal on this subject by Professor Chester Bradley was not received until long after the draft conclusion referred to above had been prepared. So far as I can see, however, all the points included in it are covered also by the draft conclusion.

8. In the documents now submitted numerous correspondents, including the two applicants referred to above, stress the importance of providing adequate safeguards to ensure that, through the use by the Commission of its plenary powers, well-established nomenclatorial usage is not disturbed in individual cases by the reversal of the Paris interpretation of *Opinion* 4. From the material supplied, it appears probable that the need for remedial action of this kind is most likely to arise in the case of names in the Mollusca. See in this connection the documents received from Professor Harold E. Vokes (Document 33/9) and from Dr. A. Myra Keen and Professor Siemon W. Muller (Document 33/23).

9. In accordance with the principle advocated by the workers referred to in the immediately preceding paragraph, it would follow that normally continuity of nomenclatorial practice would be promoted, in the case of names which have been dealt with in *Opinions*, by validating the decisions so taken, where these were not in harmony with the revised ruling. In one such case, however, namely that dealt with in *Opinion* 78 (Rocky Mountain Wood Tick), it is possible that what is required is not an automatic confirmation of the decision taken in that *Opinion*, but a complete review of the whole problem in the light of the current practice of interested specialists. It will be remembered that this particular *Opinion* has been subject to considerable criticism and that at Paris in 1948 it was specifically excluded from the decision that rulings on

individual names in previously rendered *Opinions* should be codified in schedules annexed to the *Règles*, it being decided instead that the decision taken in that *Opinion* should be reviewed by the Commission as soon as possible (see 1950, *Bull. zool. Nomencl.* 4: 338). In preparation for the review so decided upon I had a considerable amount of correspondence in the period 1948-1950 with interested specialists but eventually I came to the conclusion that it would be best to put this investigation (Commission's reference Z.N.(S.) 260) on one side until after the general problem raised by *Opinions* 4 and 78 had been settled by the Copenhagen Congress.

ANNEXE TO DOCUMENT 33/1

Draft of a Conclusion which it is suggested would be appropriate if it were decided by the International Commission on Zoological Nomenclature to recommend the Fourteenth International Congress of Zoology, Copenhagen, 1953, to amend the XX "*Règles*" to provide that the publication of a "*nomen nudum*" in a synonymy is to be held not to confer any status in zoological nomenclature upon the name so published

It is suggested that approval of the proposals submitted by Dr. Ernst Mayr (Document 33/8) and by Professor J. Chester Bradley (Document 33/25) could appropriately be secured if the International Commission on Zoological Nomenclature were to recommend the Fourteenth International Congress of Zoology:—

- (1) to repeal the decision relating to the incorporation into the *Règles* of the substance of the ruling on the status of names published in synonymies given in the Commission's *Opinion* 4, taken by the Thirteenth International Congress of Zoology, Paris, 1948, when it approved the recommendation submitted by the Commission as Conclusion 18 of the Sixth Meeting held during its Paris Session (1950, *Bull. zool. Nomencl.* 4: 145-146);
- (2) so far as concerns specific trivial names, to substitute for the provision proposed, under (1) above, to be repealed, provisions prescribing:—
 - (a) (i) that the use in manuscript of a trivial name for a given species or the publication of a given trivial name as a *nomen nudum* for a species which can be recognised from manuscript sources shall not preclude any author from publishing the trivial name concerned (in combination with the same generic name as that used in manuscript or when the name was earlier published as a *nomen nudum*) either (1) for the species to which it had earlier been applied either in manuscript or as a published *nomen nudum*, or (2) for any other species;

- (ii) that, provided that, when so published, such a trivial name is accompanied by an indication, definition or description, that trivial name shall thereby acquire availability as a name for the species to which the indication, definition or description is applicable ;
- (iii) that, save in the special case specified in (b) below, a trivial name of the class specified in (i) above shall, on being published in the conditions there specified, rank for priority as from the date on which it is so published and shall be attributed to the author by whom it is published as aforesaid ;
- (b) that, where an author publishes a trivial name which had previously existed only as a manuscript name or as a published *nomen nudum* and at the same time quotes, from manuscript sources, an indication, definition or description of the species in question prepared by the earlier author by whom the name had been used in manuscript, the trivial name in question shall acquire availability in virtue of the publication of the indication, definition or description so published, irrespective of whether the later author by whom the trivial name and the relevant extract from the earlier author's manuscript is published accepts or rejects the name which he so publishes ;
- (c) that, subject to (d) below, the publication, without an indication, definition or description, either in a synonymy given for, or otherwise in connection with, a previously established nominal species, of a trivial name which previously had existed only as a manuscript name or as a published *nomen nudum* shall confer no availability upon the name so published and therefore that a name so published shall retain its previous status of a legal non-entity as a manuscript name or, as the case may be, as a published *nomen nudum* ;
- (d) that where prior to the date of the coming into operation of (b) above, a trivial name which under that provision is to be rejected as being unavailable had been treated by specialists in the group concerned as an available name, it shall be open to such specialists to make application to the International Commission on Zoological Nomenclature for a direction that the trivial name in question be accepted as an available name and it shall then become the duty of the said Commission to direct whether or not the name in question is to be accepted, provided that in every such case public notice of the receipt of the application shall be given by the Commission in like manner as that prescribed for applications involving the possible use of the Commission's plenary powers and that a period of not less than

twelve months shall have elapsed from the date of the giving of public notice before a decision is taken by the Commission on the application in question ;

- (3) that the provisions specified in (2) above in relation to specific trivial names shall apply also :—
 - (a) to any generic name published in the synonymy of the name of some other genus ; and
 - (b) to any generic name published in combination with a specific trivial name where, on the occasion of the publication of that generic name, the specific name comprising it is cited only in the synonymy of the name of some other nominal species ;
- (4) to direct that nothing in any *Opinion* rendered by the Commission prior to July, 1948 is to be taken as containing any interpretation of the provisions in regard to the matters aforesaid then inserted in the *Règles* and as subsequently amended.

DOCUMENT 33/2

By J. E. COLLIN (*Newmarket, England*)

Letter dated 16th November, 1950

REFERENCE : Z.N.(S.)372

Thanks for your letter of the 29th October, but you realise of course that it is not an answer to the question whether the principle embodied in *Opinion 4* is contrary to the fundamental laws of Zoological Nomenclature which form the basis of Article 25 of the Rules, and also a contradiction of *Opinion 1*.

I give you credit for not being deceived by the sophistic nature of *Opinion 4*, which conceals the two facts, (1) that a publication of the recognition of an undescribed museum specimen to which a MS. name has been given does *not* mean the adoption and publication of that name as "that under which a species was first designated" (as required under Article 25), and (2) that a museum specimen is *not* a valid indication recognition of which validates the name given to it, *unless and until* the name *is* so adopted and published.

A point apparently overlooked is that there is nothing peculiar about "MS. names". Every term selected as a proposed name for a genus or species of animal is a MS. name before being validly brought forward as the name by which that genus or species is to be known. *Opinion 4* therefore constitutes an *exception* to the ruling of Article 25, which if added in the new Rules must read: "Nevertheless a MS. generic or specific name applied or attached to a museum specimen or specimens can become a valid name, *without* being adopted and validly brought forward as the designation under which a genus or species was first published, upon the publication in a generic or specific synonymy of an identification of the museum specimen or specimens to which such generic or specific name was applied or attached, and the name shall be credited to the author who published the identification, and shall date from the date of such publication".

Actually of course no "term" *can* become a specific or generic "name" in zoology until validly brought forward as the proposed or adopted designation of an animal or group of animals believed to require such a name, a fact apparently entirely overlooked when *Opinion 4* was drafted. As a consequence *Opinion 4* is impossible even as an exception to Article 25.

If *Opinion 4* contradicts Article 25, and is impossible as an exception, all MS. names supposed to have been validated under that *Opinion* must be considered invalid because they have not been proposed and published as laid

down in Article 25. I therefore suggest that your proposed questionnaire should take the form of an enquiry into the proportion of generic and specific names validated under *Opinion 4 and now in common use* which would disappear as synonyms if the date of their validation must be changed from when they were first printed in synonymy to when they were first adopted as the designation of a genus or species believed to require a name.

I consider it really important that this matter should be cleared up before the new Rules are published.

I enclose a separate note on what I believe to be the correct conclusion in regard to the selection of a type species for a genus the MS. name for which was published in the synonymy of a validly named genus.

ENCLOSURE TO MR. COLLIN'S LETTER OF 16th NOVEMBER, 1950

On type selection in the case of a MS. generic name published in the synonymy of a validly named genus

It is not surprising that the International Commission on Zoological Nomenclature should have experienced some difficulty in arriving at a decision on the correct selection of a type species for a genus supposed to have been validly founded by the publication of its MS. name in the synonymy of a validly named genus. A similar question of course arises as to the type *specimen* of a species the MS. name of which was published in the synonymy of a validly named species. It is surprising however that no one thought of *looking for the cause* of the difficulty. If this had been done it would have become obvious that the difficulty in each of these cases arises solely from the fact that neither a MS. generic name, or a MS. specific name so published, is one brought forward as the proposed designation of a genus, or of a species, believed to require a name. Actually both are so published as records of the identification of certain museum specimens standing under MS. names in a collection. These names are quoted as *MS. names*, not as names to be used as the designation of the museum specimens, or of any other specimens. Hence they remain "quoted MS. names", without status in nomenclature, and a selection of type is neither necessary nor possible. Of course directly such a MS. generic or specific name *is adopted as the designation of a genus or species* believed to require a name, and republished in accordance with the provisions of Article 25, it *then* becomes a valid name, and the selection of a type a simple matter about which no question can arise.

There is nothing peculiar about "a MS. name". Every "term" proposed (but not validated) as a zoological "name" is a MS. name until published in

accordance with Article 25, *i.e.* as the proposed or adopted name by which a described, defined, or validly indicated genus or species of animal is to be known. The words "that name" in the first paragraph of Article 25 (as well as the words "this name" in the second paragraph) refer to *any* MS. name, including one published in a synonymy, and any one must be that "under which a genus or species was first designated" in order to qualify as a valid name.

DOCUMENT 33/3

By **P. C. SYLVESTER-BRADLEY**, B.Sc. (*University of Sheffield, England*)

Letter dated 16th December, 1950

Editorial Note : The four answers given by Mr. Sylvester Bradley refer to the following four questions asked in the note (published in 1950 in the *Geological Magazine* and in January, 1951, in the *Journal of Paleontology*) in which I appealed to palaeontologists for information regarding current practice in regard to names published in synonymies and for advice on the question whether any change in the *Règles* was needed :—

- (1) Do you in your work accept as an available name (that is, as a name possessing rights under the Law of Priority and the Law of Homonymy) a trivial name originally published in a specific synonymy ?
- (2) Do other specialists in your field treat such a trivial name in the same way that you do ?
- (3) In what branch of the Animal Kingdom are you a specialist ?
- (4) (*To be answered only by those specialists, whose answer to Question No. (1) is "yes"*) : Do you consider that confusion and name-changing would result in your special field if the *Règles* were altered in such a way as to render unavailable a trivial name published in a specific synonymy ?
- (5) (*To be answered only by those specialists, whose answer to Question No. (1) is "no"*) : Do you consider that confusion and name-changing would result if the present rule that a trivial name published in a specific synonymy, thereby acquires availability, were to be strictly applied to specific names in your special field ?

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YOUR ENQUIRY IN THE " GEOLOGICAL MAGAZINE "

1. Yes, I would do so.
2. I don't know. So far as I know the question has never arisen.
3. Recent and fossil ostracoda.
4. No.

DOCUMENT 33/4

By **W. J. ARKELL**, M.A., D.Sc., F.R.S.
(*Sedgwick Museum, Cambridge University, Cambridge*)

Letter dated 16th December, 1950

Editorial Note : For the text of the questions answered by Dr. Arkell in the following letter, see the Editorial Note to Document 33/3.

ARTICLE 25. YOUR LETTER IN THE " GEOLOGICAL MAGAZINE ", p. 442

I can only recall one case of this sort in my field, fossil ammonites.

In their *Pal. Soc.* monograph, 1851-3, on the Mollusca of the Great Oolite, Morris and Lycett described and figured an ammonite as *Ammonites braikenridgii* Sowerby. In the synonymy they put *Ammonites triptolemus* Bean MS., which they found on a museum label. When revising the nomenclature of Morris and Lycett's monograph with L. R. Cox (Cox & Arkell, 1950, Survey of the Mollusca of the British Great Oolite Series, Palaeontographical Soc., part 2, p. 97), I adopted the name *Stephanocera triptolemus* (Morris & Lycett) as the valid name of the species. But it was and is also the only available name, so there is no question of passing over a later name.

For what they are worth, my replies to your questions are :—

1. Yes.
2. Don't know, but should imagine so. Cox did not demur.
3. Fossil cephalopods, and to a lesser extent other mollusca.
4. Yes. I consider that any special cases of " hardship " should be brought before the Commission for suspension if necessary. Case where a new name would be required given above.

DOCUMENT 33/5

By JOHN T. ZIMMER

(*American Museum of Natural History, New York*)

Letter dated 30th January, 1951

Dr. Ernst Mayr has shown us his proposals for the elimination by the International Commission on Zoological Nomenclature of the provisions of *Opinion 4* and *Opinion 78* whereby *nomina nuda* cited in synonymy are thereby validated as of that date as exact synonyms of the names under which they are cited, regardless of their possible origin in distinguishable material that may later be properly diagnosed or regardless of erroneous identification of any other sort.

While I may disagree with one or two of the points used by Dr. Mayr in his exposition of the case, I would say that I am in accord with his main thesis and join with him in the belief that *nomina nuda* cited in synonymy and not accepted by the author so citing them should have no standing at that point in their history but remain *nomina nuda* until such time as they are properly proposed with their own particular diagnosis. Given such diagnosis, the name may be rejected by the publishing author as a synonym of an earlier name, or it may be accepted as distinct, but at least it will have identifying characters supplied.

This should not preclude an author proposing several alternative names at the same time for a single concept, all of which would be available names. I have encountered such conditions rarely, but they are distinct from *nomina nuda* and there should be no confusion.

It might be advisable, as I believe Dr. Mayr suggested, to place certain names, that would be affected by the proposed change, on the *Official List* to avoid further confusion. In any case, you may record my backing of the proposal to invalidate *nomina nuda* cited in synonymy.

DOCUMENT 33/6

By **GEORGE S. MYERS**

(Natural History Museum, Stanford University, Stanford, California, U.S.A.)

Letter dated 5th February, 1951

From Dr. Ernst Mayr I am in receipt of a mimeographed memorandum, dated "Jan. 1951," entitled "The Status of *Nomina nuda* Listed in Synonymy". This is a revision of a similar memorandum, received some time ago from Dr. Mayr, this revision having been occasioned, he tells me, by comments I made on the original. As Dr. Mayr's document now stands, I am in strong agreement with it.

I wish to point out especially the last paragraph on p. 1 of the memorandum, in which Dr. Mayr refers to *Opinion* 53. If the provision there made in relation to names published by their own authors in synonymy, for the acceptance of such names is not recognised, then confusion will result, for names of this type or origin are widely accepted and current. I refer not only to my case of *Hyla septentrionalis* Dumeril et Bibron, concerning which I send you a reprint of my paper referred to by Dr. Mayr, but also to a much more serious case, the generic name *Eleutherodactylus* Dumeril et Bibron 1841.

The largest genus of American frogs, containing now close to 100 species, was known for a long time as *Hylodes*, a generic name the type of which does not belong to the genus. In 1904, when there were not over 50 species described, Stejneger corrected the application of *Hylodes* and pointed out the availability of *Eleutherodactylus*, and this name has become thoroughly embedded in the literature through usage in almost every paper that has mentioned this largest of all American frog genera.

There are other instances of names now current which were first published by the authors of these names, but in the synonymy, in herpetology and ichthyology, and to disturb them would be most unfortunate.

This matter is also being referred to the Committee on Zoological Nomenclature of the American Society of Ichthyologists and Herpetologists, concerning the recent appointment of which I believe I informed you. The Chairman of this Committee is William I. Follett, J.D., Curator of Fishes, California Academy of Sciences, Golden Gate Park, San Francisco, California.

DOCUMENT 33/7

By **ROUSSEAU H. FLOWER**

(University of the State of New York, New York State Science Service, New York
State Museum, Albany, N.Y., U.S.A.)

Letter dated 13th February, 1951

Editorial Note : For the text of the questions answered by Dr. Flower in the following letter, see the Editorial Note to Document 33/3.

Concerning Z.N.(S.) 372, I beg to present the following response to your questions :—

1. No. I know of no case in my files where this has been done.
2. Yes.
3. The field is Paleozoic cephalopods, and Nautiloidea, not restricted to the Paleozoic.
5. I feel strongly that the present rule will cause more confusion than it will eliminate.

I have one case which would be affected by this rule, a situation involving some other matters. Its history is briefly as follows :

Phillips in 1841 (Figures and descriptions of the Paleozoic fossils of Cornwall, etc., p. 115) was the first to validate *Cyrtoceras ornatum*, a manuscript species of Goldfuss 1832, which, from previous citations, had no description and is consequently not available as of that author or date.

Phillips based his brief description upon a drawing made by M. De Verneuil (published in the next year), based upon the original specimen in the Goldfuss collection. However, Phillips in 1841 attributed British material to this species *with question* saying, "The condition of the venter does not admit of strict comparison". He further indicates doubt as to the identity of the British British material by his heading :

Cyrtoceras ornatum (?) pl. 45, fig. 217. The illustrated specimens are of the British material. The matter would be simple, were it not that Phillips adds "In my catalogue it (the British form) was named *C. foliaceum*".

The entire description is quoted below to avoid confusion :—

217. CYRTOCERAS ORNATUM(?), pl. 45, fig. 217.

Syn.—Goldfuss—(name in collection at Bonn).

Character.—General figure incurved, and tapering; surface ornamented by many longitudinal ridges and furrows, crossed by numerous waved elevated striae, and at distant intervals by large prominent rugged annulations; section (?); septa rather deeply concave; siphuncle near the dorsal line.

It is to M. DeVerneuil that I owe this reference to Goldfuss's name. The drawing which he showed me exhibits a character on the back, for which the specimens I have seen from Devon are not sufficient to allow of strict comparison. In my catalogue it was named *C. foliaceum*.

Locality.—In South Devon: Newton Bushel.

The drawing of Mr. de Verneuil is undoubtedly that which was published a year later (*Geol. Soc. London, Trans.*, 2nd series, vol. 6, pl. 28, fig. 5, 5a, 5b).

D'Orbigny considered the British and German forms distinct and proposed the name *Gyroceras goldfussi* for the German form. (1850, *Prodrom de Paleontologie Systematique*, p. 4.)

This selection is abrogated by the international rules since it implies selection of the British form as a lectotype. The rules clearly state that selection is not permissible from among specimens attributed to the species with doubt.

Whidborne (1889, Monograph of the Devonian fauna of the south of England, *Palaeontographical Society Monograph*, pp. 92, 94) decided that the British and German forms were distinct, that the British one was the same as the species he described as *Gyroceras praeclarum* (1889, *Geol. Mag.*, decade 3, vol. 6, p. 29). This name has since been in general use.

Under the present rule, it would be necessary to restore *Cyrtoceras foliaceum* and regard *Gyroceras praeclarum* as a junior homonym which should be suppressed.

In this case observance of the present rules will require the use of a name which has been proposed only tentatively, based upon a rather poor type, in favour of a name based upon better preserved and more complete material. The situation in regard to specific identification which is brought up is delicate, and would be removed by a reversal of the present ruling or an action by the Commission arbitrarily invalidating *Cyrtoceras foliaceum* Phillips, an action which I would rather not request of this already sadly overworked body.

There is one further perplexity on which I am not clear. The case I have cited involves several other aspects of the rules than the one point on which you requested the views of various workers. To put it simply :

Mr. X in 1900 discusses a species *A. a.* Mr. Y in 1901 discusses the species listing as a synonym his own manuscript species, *A. b.* If species *A. a.*, X, 1900, is preoccupied and *A. b.*, Y, 1901, replaces it automatically, what restrictions apply to the type species ? Should only the material of Mr. Y be available for the selection of a lectotype ? It would seem so.

In my field I do not know of other cases, but I feel that this is because the availability of such names which appear in print first, and usually, only, as junior synonyms, has not been generally recognised. It may easily cause changes in the names at present in general use, not a few of which are type species of large and widely-known genera, which will make the confusion greater.

DOCUMENT 33/8

By ERNST MAYR (*American Museum of Natural History, New York*)

Enclosure to a letter dated 19th February, 1951

Note: *The present document was first communicated informally to the International Commission by Dr. Mayr under cover of a letter dated 20th August, 1950.*

THE STATUS OF "NOMINA NUDA" LISTED IN SYNONYMY

A *nomen nudum* is a scientific name proposed after 1757 in such a manner as not to fulfil the requirements of Article 25 (see also *Opinion 126*). The qualifications of *nomina nuda* were changed 31st December, 1930, with the revision of Article 25c which now requires that descriptions of new species or genera must "differentiate or distinguish the genus or the species from other genera or species."

A *nomen nudum* is without nomenclatorial standing and does not affect the validity of other names. If the same specific or generic name is subsequently validly proposed, this name is not in any way affected by the prior *nomen nudum*. If a valid description for the *nomen nudum* is published at a later date, the name and the authorship date from the moment of publication of the validating subsequent description. As it is often stated, "once a *nomen nudum*, always a *nomen nudum*." A *nomen nudum* never pre-empts the subsequent usage of the same name.

Bull. zool. Nomencl., vol. 10 (July, 1953).

The status of *nomina nuda* (manuscript names) was regulated by *Opinion 4* (1907): "Manuscript names acquire standing in nomenclature when printed in connection with the provisions of Article 25 and the question of their validity is not influenced by the fact whether such names are accepted or rejected by the author responsible for their publication." There is nothing particularly injurious to the stability of nomenclature in this ruling, although the wisdom of the "rejection" clause of the *Opinion* has often been questioned, and indeed it contradicts all common sense that a scientist should become the author of a name merely by stating (in the proper context) that it is not a valid name.

Opinion 4 has been applied by most taxonomists to names published with the standard provisions of Article 25, namely, an adequate description, an illustration, or a bibliographic reference to a previously published description or illustration. Great confusion has been created by extending the interpretation of the word "indication" in Article 25 in its application to *Opinion 4* to include the listing of manuscript names in the synonymy of valid names. This principle was officially stated for the first time in the discussion by Stiles in the comments with *Opinion 78* (published 9th February, 1924) and shall, for short, be referred to in the subsequent discussion as the Stiles Ruling (or interpretation). Contrary to Stiles's own opinion, this interpretation is not the unavoidable consequence of *Opinions 4* and 53, as we shall presently explain. This seemingly innocuous ruling is a Trojan horse of gigantic dimensions. It not only threatens the stability of nomenclature in the most serious manner, but it also will lead—if consistently applied—to a whole new philosophy of nomenclature. Furthermore, it violates several of the articles of the International Code explicitly or implicitly, as well as several *Opinions*. A detailed discussion of this ruling seems therefore urgently needed.

The Stiles interpretation of *Opinion 4* is based on the belief that the listing of a name in synonymy is equivalent to a valid indication as demanded by Article 25. It appears that this is contrary to the best accepted practice. "Indication", as hitherto interpreted, is either the proposal of a new name with a bibliographic reference to a previously published diagnosis or description or the unambiguous proposal of a valid name to replace a previous name which is considered unavailable. The mere expression of an opinion, namely, that a name is a synonym of another name, is not such a proposal in the sense of Article 25. The International Commission has decided three times previously that invalid names do not acquire validity by listing in synonymy

- (1) for pre-Linnaean names (*Opinion 5*)
- (2) for misspellings
- (3) for infrasubspecific names.

In view of this multiple clear-cut precedent it is confusing to make an inconsistent exception for *nomina nuda* (manuscript names).

The ruling also conflicts clearly with two of the articles.

Article 21 states, although in a rather round-about manner, that the author of a name is the person who is responsible for it. If an author records a name specifically as that of another author he certainly thereby renounces clearly the responsibility for this name and it makes no difference whether or not that name has nomenclatorial validity. It would be circular reasoning of the worst sort to use Article 21 to prove that listing in synonymy is an indication in the sense of Article 25.

Reference must be made here to *Opinion 53*. This *Opinion* deals with manuscript names which by their own authors are proposed as alternative names of valid names. In such a case the author assumes responsibility for the nomenclatorial disposition of his own manuscript name. It can therefore be argued, and this has apparently been the majority opinion, that such cases satisfy the provisions of Article 21. This, for instance, has been the interpretation given by Deignan (1948) for *Ixos pyrrhotis* Hodgson and by Myers (1950) for *Hyla septentrionalis* Dumeril and Bibron. The interpretation sometimes leads to complications, but it does not lead to the inevitable confusion created by extending the ruling to validation of manuscript names of other authors.

The Stiles Ruling also violates in all of its important applications the principle embodied in Article 31, namely, that a name "which undoubtedly rests upon an error of identification cannot be retained." This will be shown presently.

It is interesting to note that in *Opinion 53*, which deals specifically with manuscript names in synonymy, a name is definitely shifted from one species (*Syngnathus conspicillatus* Jenyns) to another (*Halicampus grayi* Kaup) because it could be shown that the synonymising of *Halicampus grayi* Kaup with *Syngnathus conspicillatus* Jenyns was based on a misidentification. In other words, it contradicts the interpretation of *Opinion 78* which insists that a manuscript name becomes automatically an alternative name of any valid name in whose synonymy it is placed.

Much more serious than these explicit or implicit violations of five specific articles or previous rulings is the fact that the ruling constitutes a definite infringement on the domain of taxonomy by nomenclature. It has, up to now, been considered axiomatic that the International Commission cannot rule on matters of zoology. The Stiles interpretation of *Opinion 4* usurps rights for nomenclature that rightfully belong to taxonomy. The Commission has confused zoological species and nomenclatorial species-names. Its ruling implies that not only the *nomen nudum* has no nomenclatorial existence (a ruling with which all of us agree), but also that not even the species exists on which the manuscript name or *nomen nudum* is based. This is an error, based on a misunderstanding of the zoological basis of *nomina nuda*. A manuscript

name, even if invalid according to Article 25, has an author and is based *in most cases on a definite, concretely labelled type specimen* (see also *Opinion 53*). Although non-existent nomenclatorially, the identity of the zoological basis of the name is in many cases not at all in doubt. This is true particularly in the many cases in which the author of the manuscript name has subsequently published an adequate diagnosis.

The type of a manuscript name serves as its onomatophore (name bearer) (Simpson, 1940) and for *this* function it is quite immaterial whether or not the name has nomenclatorial validity. It is the axiomatic basis of the entire type method that the onomatophore can never be changed. If a name could be made valid merely by placing it in synonymy, the grave danger could arise of a name being shifted to a new onomatophore. This is a logical impossibility and a zoological error. The danger of this is apparent as soon as it is realised how often names are quoted in the wrong synonymy. If the International Commission could completely destroy the type of the original manuscript name and all manuscript notes about it, no confusion might result. However the type with the type label continues to exist and a valid description with this name may be published sooner or later, leading to obvious confusion and homonymies.

Synonymising a species with another is a *zoological* action based on a *taxonomic* decision. The rules of nomenclature decide only what to do with these names after such a decision has been made. Synonymising is always a subjective action except in the rare cases where several names are based on a single type specimen. As a subjective action it is subject to error and must be reversible. Normally a name can be removed from the synonymy as soon as it is determined that the type does not belong to the same zoological unit as the name with which it was synonymised. The Stiles Ruling on *Opinion 4*, by supplying a second type specimen, attempts to produce an irrevocable synonymy. It cannot be corrected even if it is determined that the original action of synonymising was based on an error. An "indication" (as required by Article 25) must be something objective, permanent, unequivocal, and irreversible. The placing of a name in synonymy is something subjective, often temporary, often arbitrary, and always potentially reversible. It is therefore obvious that the listing of a name in synonymy should not be allowed to qualify as an "indication" as required by Article 25.

The implication of the Stiles interpretation that a manuscript name has no zoological basis has the superficial appearance of legal correctness, but it is contrary to taxonomic principles and to common sense.

A rule which permits the shifting of a name bearer opens the door wide for an endless amount of name changing resulting in much nomenclatorial confusion. That this danger is not imaginary is evident from recent happenings in ornithology. Literally dozens, if not hundreds of well-established bird names are threatened with change. Our present rules of nomenclature were adopted in

1905. It is obvious that taxonomists between 1758 and 1850 did not always obey rules that were to be adopted 50 to 150 years later. For instance, Temminck, Lichtensten, Bonaparte, S. Müller, Hodgson, and many other ornithologists of the first half of the last century labelled new species as such in their collections and published the names as *nomina nuda* in local lists, lists of duplicates, etc. After such an informal and, of course, invalid introduction into the literature, sometimes 10, sometimes 50 years passed until these names were supplied with a description and introduced into the literature in a valid manner. It has been customary in ornithology in accordance with *Opinion 4* to cite such names from the date (and with the author) of the final valid publication. The earlier publication of these *nomina nuda* was justly ignored. However, many of these names were listed (often very erroneously!) as synonyms of other species between their original invalid and the subsequent valid publication. It has been customary to ignore these misidentifications. If we were to follow the Stiles interpretation of *Opinion 4* it would mean that a considerable number of familiar animal names will have to be changed. A beginning of this has already been made by some *virorum novarum cupidi*! For instance, Mittleman (1950) wants to place the well-known name of the large Cuban Tree Frog *Hyla septentrionalis* in the synonymy of the Java species *Rana chalconota* Schlegel and replace it by the name *Hyla insulsa* (Cope). However, as Myers (1950) shows, Schlegel's reference of the name *Hyla septentrionalis* was erroneous, and using similar arguments as those given above, Myers rejects the propriety of applying *Opinion 4*.

Alauda dulcivox Hodgson 1844 was a *nomen nudum* when first listed. In 1858 the name was listed by Horsfield and Moore (Cat. Birds Mus. E.I. Co., vol. II, p. 466) in the synonymy of *Alauda arvensis* L. In 1873 the name was introduced for the first time validly into the literature by Brooks (Stray Feathers, vol. 1, p. 484) who supplied a description and the name has since been quoted by most authors as of Brooks 1873, disregarding Hodgson's *nomen nudum* or the erroneous synonymy of Horsfield and Moore. Both Meinertzhagen and Vaurie in their recent revisions of *Alauda arvensis* have accepted the name *dulcivox* as of Brooks 1873 (contrary to *Opinion 78*).

Worse are the cases where manuscript names of subspecies are listed by subsequent authors under the binominal designation of the polytypic species. If the new interpretation of *Opinion 4* were accepted they would become alternative names for the nominate subspecies. The absurdity of such a procedure is evident.

For instance, when working on his *Oceanic Birds of South America* (in the mid-20s) R. C. Murphy described (in MS.) a new subspecies of *Oceanites oceanicus* as *chilensis* and marked a specimen in the American Museum of Natural History (New York) as a type. Without supplying a description (or mentioning the author) Alexander, 1928, *Birds of the Ocean* (p. 86) lists the

subspecies *chilensis* for the islands off the west coast of South America. In 1934 G. M. Mathews quoted Alexander's name and reference, placing it in the synonymy of the nominate race. This would make the name an alternative name for *oceanicus* under the Stiles opinion. The formal description of *chilensis* was published by Murphy in 1936. Nearly every author since including Mathews in a subsequent publication, has accepted the name *chilensis* as dating from 1936 (but see Sheard, 1943, for dissenting opinions). To use the name *chilensis* as a synonym for the Atlantic Ocean subspecies *oceanicus* would be obvious nonsense.

An even more injurious example concerns the name *Eurystomus orientalis calonyx* Sharpe 1890, a name used for more than 50 years for the exceedingly common, widespread, and familiar Dollarbird of eastern Asia. The name had first been proposed (as *calornyx*!) by Hodgson in 1844 (*nomen nudum*) and this *nomen nudum* was cited under the species name *E. orientalis* by Gray 1848. From 1890 to 1942 the name *calonyx* Sharpe has been used in dozens, if not hundreds, of publications. In 1942 the subspecies was renamed *abundus* by Ripley, at the suggestion of J. L. Peters, as being preoccupied by *calornyx* Gray 1848! This ill-advised change of the well-established name *calonyx* has fortunately not been accepted by several recent writers, although it would be correct if the Stiles interpretation of *Opinion 4* were accepted.

These are merely a few examples which could be multiplied many times. They show, however, what is involved. Cases have been recorded in entomology where the same manuscript name has been listed in the synonymy of four different species. According to the Stiles interpretation this name might be an alternative available name for each of the four different species!

There is one other serious objection to this ruling: it places an intolerable burden of bibliographic research on every author who is on the point of proposing a new species. In order to avoid making a homonym he must not only eliminate from usage all prior validly proposed names, but he must also make sure that the combination has never before been used as a *nomen nudum* and been quoted in synonymy. Since *nomina nuda* are listed only by some cataloguers and monographers it means that every single publication of the group back to Linnaeus must be consulted that has ever used synonymies! Instead of being able to devote his time to biological and taxonomic research, *Opinion 4* (in its extended interpretation) would henceforth force the taxonomist to waste all his time on bibliographic research. This is a particular burden for the entomologist because, if listing in a synonymy is considered a valid indication in the sense of Article 25, nomenclatorial recognition is at once also given to all the names of infrasubspecific forms which nomenclatorially have been considered up to now equivalent to *nomina nuda*. The inevitable consequence of the new interpretation is that the name of every aberration, seasonal form, polymorphic variety, age stage, and other individual variant, becomes a valid substitute name for the species or subspecies under which it

has been quoted. Furthermore, it pre-empts the use of these names for all the future for any other species or subspecies of the genus. I hardly need to describe the nomenclatorial chaos that would be the result of this interpretation.

Conclusion

It is obvious from this discussion that the Stiles interpretation, accepted by the International Commission in *Opinion* 78, to consider the synonymising of a manuscript name as a valid indication in the sense of Article 25 is a serious mistake and should be corrected at once.

(1) It is in conflict, implicitly or explicitly, with the provisions of Articles 21 and 31, as well as with repeated rulings in other cases that an invalid name (*e.g.*, pre-Linnaean name, variety name, etc.) is not validated by being placed in the synonymy of a valid name.

(2) Listing another author's manuscript name in synonymy is not an objective indication and is based on a misconception of the zoological basis of manuscript names and, in fact, constitutes often an attempt to shift this basis (the "onomatophore" of the name).

(3) The ruling places an intolerable burden of bibliographic research on the taxonomist who would henceforth not only have to list all valid names, but also all occasions where a *nomen nudum* has been listed in the synonymy.

(4) Since many of the manuscript names were subsequently validly published but the preliminary quoting in synonymy was erroneous, the new ruling threatens hundreds of well-established names. It is thus a ruling which "clearly results in greater confusion than uniformity".

The total weight of this conclusion is not seriously lessened even if one or the other of the preceding arguments is held incorrect or inadequate. Even if several of the above points were held inadmissible, the remainder would still carry enough weight to necessitate an immediate revoking of the unfortunate ruling of *Opinion* 78 extending the interpretation of *Opinion* 4.

Recommendations

It is recommended that the International Commission at once repeal the ruling in *Opinion* 78 which interprets the (erroneous or not) listing of manuscript names of other authors in synonymies as a valid indication in the sense of

Article 25. Manuscript names should be added to the category of names (such as pre-Linnaean names) that have no nomenclatorial rights. No exception should be granted to their status, not even quickening in synonymy.

A manuscript name should become valid only if it is published in accordance with Article 21, that is, if it is published by a subsequent author in such a manner as to indicate that the author considers himself responsible for the name. A manuscript name, not having any nomenclatorial status whatsoever, does not pre-empt the subsequent proposal of the same specific or scientific name for a species based either on the same or on a different type specimen.

Some name changes based on the Stiles ruling have been rather generally accepted. Such names should not be affected retroactively. The Commission should decide these cases on the merits of the individual case, keeping in mind the Monaco admonition to avoid any change that "will clearly result in greater confusion than uniformity".

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DOCUMENT 33/9

By HAROLD E. VOKES

(The Johns Hopkins University, Department of Geology, Baltimore, Maryland,
U.S.A.)

(1) Extract from a letter dated 23rd February, 1951

Fortunately, I am now well on the road to recovery again and am moved to write you to comment on the communication that was published in the *Journal of Paleontology* in the "January" 1951 issue (received in my office just last week).

With regard to the problem of the use of names published in synonymy, I think that the removal of this provision would cause much confusion in the field of molluscan (particularly Pelecypod) studies, both recent and fossil. This confusion would come on the generic level, where many names, originally proposed in synonymy have been later, and for more than one hundred years, in rather general usage.

While I am aware that your correspondent who has raised the question of the status of *Opinion 4* was concerned with the matter on the specific level (i.e., the validity of trivial names so published), in my opinion the wording of *Opinion 4* clearly also involves generic names, and the problem must be considered on this level also, and it is here that our nomenclature would be most seriously affected.

I might cite the many manuscript names of the former English malacologist Leach published by various authors over a period of about fifty years of the early part of the past century. Thus Lamarck, in his *Anim. sans Vert.* mentions a number of Leach MS. names in his synonymies, and several of these have, on the generic level, been subsequently brought into use. Such, for example is the case of the genus *Thyasira* Leach MS., Lamarck, published in synonymy in volume 5, p. 492 of the above work. Today this genus is everywhere recognised as being validly proposed and is the type of the family *Thyasiridae*. For another example, outside of the Lamarck use of these names: the name *Psammophila*, used by many as a subgenus, or "section" of the genus *Lutraria*, family MACTRIDAE, was first published by Brown in 1827 in his "Illustrated Conchology of Great Britain and Ireland" in synonymy in the plate description to plate 12, figure 1 of his work. Leach's manuscript bearing this name was not published until 1852, when J. E. Gray, of the British Museum,

edited the work as a posthumous publication. (Incidentally Neave cites these later names as Leach MS., Gray, though most workers consider that Leach should be given full authorship.)

I must admit that I have had neither the time nor, as yet, the energy to attempt to follow through the entire list of pelecypod names to find out just how many would be involved in such a problem. The above, are, however, typical examples of the nature of the problem that would be raised on the generic level, if the suggestion as to the withdrawal of *Opinion 4* was to be accepted. Offhand, I am not acquainted with many trivial names that would be so effected, perhaps the malacologists at the British Museum, or the local members of the Malacological Society of London could give some information as to the extent that the adoption of Leach MS. names would affect the nomenclature of the British fauna.

(2) Extract from a letter dated 30th July, 1951

(3) In our communications of last Spring concerned with Dr. Mayr's request for a ruling on the status of specific names first published in synonymy (*Opinion 4*) I had made a point of objecting to the ruling out of such names on a broad basis because of the number of names on the generic level that had first appeared in this manner, especially in the mollusca, and particularly as a result of the apparently wide circulation of a manuscript of Leach on the mollusca of the British Isles. This manuscript was not published until 1852, after the death of Leach, but his manuscript names were quoted in synonymy by many authors, including Lamarck as early as 1818 (*Syst. anims. Vert.*), and received wide acceptance. As I recall I specifically cited the case of *Thyasira* which first appeared in Lamarck as *Thyasira flexuosa* Leach manuscript in the synonymy of *Tellina flexuosa* as it was denominated by Lamarck. Within a very few years the name *Thyasira* was being widely used (with a number of misspellings) and it has continued to be used to this day, being now the type genus of the family THYASIRIDAE, with a geologic record from the Cocene in Japan and the Oligocene in western North America.

At the time of our earlier correspondence I had not noted that this very situation had apparently been considered during the Twelfth Meeting of the Paris session and that, as phrased on p. 351, of *Bull. zool. Nomencl.*, vol. 4 :

“ THE COMMISSION agreed to recommend :—

that words should be inserted in Article 25 to make it clear that a generic name is not to be treated as having been published with an indication by virtue only of its having been published as a generic component of a specific name cited in a synonymy given for a nominal species, and accordingly that a generic name so published does not thereby acquire any status in zoological nomenclature ”.

This would seem to be conclusive, but it is bound to cause much confusion, or at least uncertainty and inconvenience, for I believe that a list of perhaps twenty Leach names alone, can be drawn up, that will have to be revised, so far as long-accepted status is concerned. In the light of the wording of the statement in the *Bulletin*, I would assume that the use of the name in the synonymy does not serve to pre-occupy the use of that name and that the first subsequent author, who uses the name outside of a synonymy may be assumed to have validly established it. If this later author uses it in association with the species which is currently accepted as the valid type, then little other than inconvenience will have been caused ; if however, the presently accepted type species is not available, it may be necessary to request the use of the plenary powers.

DOCUMENT 33/10**By CHARLES H. BLAKE**

(*Massachusetts Institute of Technology, Department of Biology, Cambridge,
Massachusetts, U.S.A.*)

(1) Letter dated 28th February, 1951

Editorial Note : For the text of the questions answered by Professor Blake in the following letter, see the Editorial Note to Document 33/3.

Referring to your communication in the *Journal of Paleontology*, January 1951, No. Z.N.(S.)372, I would like to reply, not so much as a specialist, but as one for whom names are tools rather than property clothed with vested interests. I will, therefore, give you conclusions on most of the numbered paragraphs and then answers to the questions in paragraph 9.

1. In the instance cited in paragraph 2, the Commission should use its plenary powers.

2. I agree to the Conclusion in paragraph 4.

3. In my own experience, this type of case is sufficiently rare so that variations of practice from one group to another may not be statistically significant. Personally, in any event, I regard such synonyms as validly published and available. There may be no clear answer to the issue raised in paragraph 6.

4. I agree to the conclusion in paragraph 7 with the further proviso that clearly the names must remain available for the action of both the Law of Priority and the Law on Homonymy.

5. To the questions in paragraph 9, I would give the following replies :—

(1) Yes.

(3) My systematic and nomenclatorial work has been mostly with living Crustacea and living insects.

(4) Probably not.

6. I am pleased to see, in your communication, emphasis placed on stability as opposed to uniformity.

7. Valid conclusions in this sort of case might become quite different in the future if authors took to publishing needless, manufactured synonyms to cover the possible unavailability of the names they really publish.

(2) Extract from a letter dated 9th April, 1952

It would appear as a consequence of some correspondence recently with Dr. Ernst Mayr that I arrived at a somewhat different conclusion in reading your communication in the *Journal of Paleontology* for January 1951 than you intended. The communication in question speaks of manuscript names. Mayr at least believes that by that nude names were intended. To me at least those are two quite different classes of names. Manuscript names from my point of view are neither nude nor clothed, they in effect have no existence until published and when published they take whatever meaning they have from the circumstances surrounding their publication. Therefore, a manuscript name in my interpretation when published in synonymy is automatically a synonym of the specific name adopted by the publishing author and my answer rested on that premise. It would certainly be rare that an author would cite a manuscript name and then specifically state that it was not synonymous with the species name under discussion. He would be far more likely to make no mention of it at all. On the other hand, nude names which are already published but without description or indication of their meaning an author might either accept as valid synonyms of his own name or reject, since nude names have sometimes been discussed and assigned variously by different subsequent writers. I would, in fact, agree with the essence of Mayr's view that the validity of a nude name as a synonym is something to be determined by the author who cites the name and if he states that it is not a synonym then it would seem improper for any subsequent worker to accept it as a valid synonym on the basis of that author who expressly rejects it. A subsequent worker could, of course, accept it on his own motion but in that case the allocation would date from him and not some prior and rejecting author.

DOCUMENT 33/11

By **BROOKS F. ELLIS**

(Department of Micropaleontology, The American Museum of Natural History,
New York)

Letter dated 14th March, 1951

In the course of preparing sheets for the Catalogue of Foraminifera, which we publish, the question has often arisen whether or not a particular trivial name originally published as a synonym should be considered available. Our policy heretofore has been to treat the name much as we would have treated it if it had not been published as a synonym. For example, many of d'Orbigny's names were *nomina nuda* when originally published in 1826; later, between 1900 and 1904, Fornasini republished d'Orbigny's names together with d'Orbigny's previously unpublished illustrations. Fornasini indicated in many cases that a particular name was, in his opinion, a synonym of another name already in use in the literature. Nevertheless, because d'Orbigny's name was published in connection with a figure and a brief description, we have considered each of these names available and have issued Catalogue sheets for them.

A similar situation exists with regard to some of Schellwien's fusuline names. Schellwien died before his Monograph on the Fusulines was published, and the manuscript was issued and edited by Dyhrenfurth and von Staff. They published Schellwien's names in the plate explanations, but suppressed some of them as synonyms of other names in the text.

We have regarded both of these cases, d'Orbigny-Fornasini and Schellwien-Staff, as if they were the work of separate authors. When the dead author's names are made available by publication with a description and/or figure, we have accepted them, and have indicated their suppression as synonyms, as if it were the work of the second author in a separate publication.

In other cases, where the name in question has been published as a subjective synonym, but without accompanying description and/or figure which would characterise it specifically, we have treated the name in synonymy as a *nomen nudum*. This applies particularly well to "*Lituola subglobosa* Sars," published as a *nomen nudum* in lists by Sars; mentioned, still as a *nomen nudum*, by Brady in a few publications; and eventually listed by Brady in the Challenger Report (1884) as a subjective synonym of "*Haplophragmium latidorsatum* Bornemann". There is some discussion in the text concerning the suppression of Sars' name, but nothing which characterises Sars' species. The name is therefore considered to be still a *nomen nudum* in Brady's

publication. In 1910, Cushman used Sars' trivial name, crediting it to Sars, in connection with a description and figures (not as a synonym), and we have accepted the name as available as of that date. Severe difficulties would arise if we accepted Sars' name as available on the basis of its supposed synonymy with "*Haplophragmium latidorsatum* Bornemann," for it would then become an *objective* synonym of Bornemann's name, whereas it has turned out, from examination of specimens by several specialists, that Sars' Recent species and Bornemann's fossil one are not even congeneric.

Our policy, therefore, can be stated as follows: We accept as available names which were first published in synonymy (a) if they are published as *objective* synonyms or (b) if they are published as *subjective* synonyms together with some characterising information, such as a description or (before 1931) a figure or a reference to such information elsewhere in the literature. Subjective synonyms without any characterising information are regarded as *nomina nuda*.

The policy of other workers in our field appears from the literature to be similar to ours in general. Both d'Orbigny's and Schellwien's names, for example, are universally accepted on the basis of their publication by Fornasini and von Staff, respectively. Policy with regard to undescribed and unfigured synonyms differs: Many workers, for example, accept "*Lituola subglobosa* Sars" as available by reason of its publication by Brady as a synonym of "*Haplophragmium latidorsatum* Bornemann," although at the same time they use the two trivial names for two different species which are classified in two different genera. This seems to us illogical, since the only basis for accepting Sars' name would be Brady's description and figures of what he considered to be Bornemann's species (according to modern authors, Brady's form included *three* different species).

In answer to your question No. 3, our published work is concerned only with foraminifera, both Recent and fossil.

Question 4: It would cause considerable confusion and name changing if trivial names published in a specific synonymy were to be made unavailable, if such a rule were retroactive. The specific names of d'Orbigny and Schellwien and many others would have to be suppressed. If the rule were to apply only to future names, less confusion would result.

Question 5: If all names published originally in synonymy, including subjective synonyms without any specific characterising information, were to be accepted as available as substitute names for the nominal species, much confusion would result in our field. For example, such a rule would suppress Sars' specific name, cited above, as an *objective* synonym of Bornemann's fossil species, and force renaming of the Recent species, which is widely referred to in the literature.

In short, we feel that "circumstances alter cases", but that a rule could be drawn up which would differentiate clearly between subjective synonyms and objective synonyms, the latter class becoming automatically available, the former becoming available only if published in connection with a description or (before 1931) a figure, or a reference to such information in the literature, which applies *specifically* to the name in question.

DOCUMENT 33/12

By **J. MARVIN WELLER**

(*University of Chicago, Department of Geology, Chicago, Illinois, U.S.A.*)

Letter dated 29th March, 1951

Commenting on your note in the January number of the *Journal of Paleontology* :

I do not answer your questions because I have no examples of this type in mind, but I have questions of my own to raise. These involve the implication as far as type specimens are concerned in this sort of situation.

If (1) an author describes a new species and includes in the synonymy, along with other names that have been wrongly used for mis-identified similar specimens, the manuscript name of another author, and if (2) the new specific name that he introduced is a homonym, and if (3) the manuscript name is accepted as the valid name of this species, what is the type specimen of this species? Is it the type selected by the publishing author in spite of the fact that the manuscript author probably never saw it? Or is it the type selected by the manuscript author in spite of the fact that this has never been illustrated or described in print and the publishing author may be wrong in considering the two names to be synonymous?

Because these questions arise and because either answer appears illogical I think that publication of a manuscript name in synonymy should not validate it.

Distinction might be made between synonymic names of this type and those which have been clearly introduced as substitute names which might be valid if the preferred one proved to be a homonym. For example, I had the bad judgment, after naming a trilobite *Kaskia chesterensis*, to remark that this species (including some of the identical specimens) had been described under the manuscript name of *Phillipsia okawensis* (*Jour. Paleo.* Vol. 10, p. 461). Uncertain and borderline cases, however, almost certainly will be found, so it seems to me the only safe procedure may be to outlaw all manuscript names introduced in this way.

DOCUMENT 33/13

Views of workers at the

BRITISH MUSEUM (NATURAL HISTORY), LONDON

Letter received on 8th August, 1951

(Communicated by Dr. Fritz van Emden)

(Commission's Reference : Z.N.(S.)372)

Having taken cognisance of Mr. J. E. Collin's "Memorandum" on *Opinion* 4 and MS. names, Dr. E. Mayr's note on "The Status of Nomina Nuda Listed in Synonymy" and your own recent article in *Journal of Paleontology*, vol. 25 (1951) p. 118, the undersigned support strongly any effort at abolishing the ruling according to which all *nomina nuda* listed and published in synonymy are validly published zoological names. We feel that in the great majority of cases such a binding rule would create greater confusion than uniformity, and that therefore as a general rule these names should be considered as still-born, without prejudice to the sanctioning of exceptions by the Commission where such names are found to be in general use, so that in these special cases their rejection would create greater confusion than uniformity.

As far as material answering the questions raised in your own discussion of the subject is readily available, this will be typed out separately and attached to this letter.

T. C. S. Morrison-Scott
H. Oldroyd
Paul Freeman
Fritz van Emden
Elwood C. Zimmerman
H. W. Parker
M. Burton
T. H. C. Taylor
H. Dighton Thomas
L. R. Cox
H. M. Muir-Wood
Guy A. K. Marshall
J. Balfour-Browne
E. B. Britton
R. D. Pope
D. J. Atkinson
Theresa Clay
W. E. Swinton

W. J. Rees
A. K. Totton
E. Trewavas
R. L. Coe
N. Tebble
L. Bairstow
Eroll I. White
J. M. Carvalho
W. E. China
N. C. E. Miller
J. P. Harding
K. P. Oakley
Jacubski
J. C. Battersby
S. Prudhoe
A. K. Clark
G. O. Evans
Cameron D. Ovey

D. W. Tucker

Annexe 1 to Document 33/13

By **G. J. KERRICK** (*Hym. parasitica*)

I have no experience of any such case, and do not at the present time wish to take a view of the problem.

Annexe 2 to Document 33/13*

By **G. E. J. NIXON**

- (1) No.
- (2) Cannot say offhand but hope so.
- (3) Hymenoptera : Braconidae and Serphoidea.
- (5) Only to a very small extent.

Annexe 3 to Document 33/13*

By **R. B. BENSON**

- (1) I do not know of any case.
- (2) Presumably.
- (3) Hym.
- (5) Scarcely.

* For the questions to which the answers are here given see Editorial Note to Document 33/3.

Annexe 4 to Document 33/13

By LESLIE BAIRSTOW

Supplementary note in reply to Mr. Hemming's questions

I think that it would be preferable not to consider a trivial name as necessarily becoming more than a *nomen nudum* even though quoted (whether in a formally tabulated specific synonymy or otherwise) as a synonym of an established species. I am not convinced, moreover, that any change of rule would, as Mr. Hemming considers (1951, *Journ. Paleont.*, vol. 25, pp. 119-20), necessarily have to be such as to deny validity even to a name explicitly stated to be published conditionally as a possibly-needed substitute name.

Martin Simpson, 1884, "The Fossils of the Yorkshire Lias," Second Edition, p. 44, after redescribing *Belemnites laevigatus* Simpson (1866, Mackie's Geological and Nat. History Repertory, I, p. 216), added a note "If the above name be inadmissible, it may be called *B. complanatus*". *B. laevigatus* Simpson, 1866, is indeed pre-occupied by *B. laevigatus* Sieten, 1831, *Petrif. Wurtemberg*, p. 28 and pl. XXI, fig. 12. In these circumstances it seems to me proper that Simpson's name *B. complanatus* should be available for consideration even though proposed only conditionally. However, in this instance it makes no difference in practice, for already in 1876 Blake (in Tate and Blake, "The Yorkshire Lias," pp. 318-319) had proposed *Belemnites aspergillum* on a basis apparently including Simpson's material of *B. laevigatus*; whilst in any event the name *B. complanatus* Simpson, 1884, is itself pre-occupied by *B. complanatus* Raspail, 1829 (*Hist. nat. des Belemnites*, *Ann. sci. Observ.*, p. 313 (43) and pl. VII, figs. 63 and 64).

DOCUMENT 33/14

By ERNST MAYR

(The American Museum of Natural History, New York)

Extract from a letter dated 11th February, 1952

**COMMENT UPON A POINT RAISED BY PROFESSOR HAROLD E. VOKES
IN A LETTER DATED 23rd FEBRUARY, 1951 (SEE DOCUMENT 33/9)**

I have once more studied the case of the genera mentioned by Professor Vokes and found that there are actually two situations involved. Only one of the two is comparable with the situation with respect to specific trivial names covered by my application. If we have a generic name under which another generic name (previously unpublished) is mentioned without the mentioning of any species, the situation is precisely that as the one to which I refer in my petition with respect to specific trivial names. However, since a generic name is defined by the included species or, at least, it was so until 1930, one cannot speak of a *nomen nudum* if definite specific names are cited with the generic name quoted in synonymy from a manuscript. I have checked Dr. Vokes' citations and found that he is referring to the second class of generic names. Even here great difficulties would be created by *Opinion 78*. According to *Opinion 78* such generic names would become automatic synonyms of the genus under which they are listed. Consequently, precisely the opposite of Professor Vokes' wishes would come true. The acceptance of *Opinion 78* would lead to the rejection of the names which Professor Vokes would like to save, while the proposed rejection of *Opinion 78* would lead to their saving. It would seem to me that you should make a clear distinction in your presentation between the two kinds of generic names, otherwise there is a risk of confusion arising. Professor Vokes has not been referring to true *nomina nuda*, although the somewhat unfortunate term "manuscript names" includes both groups of names. It was for this very reason that I have chosen the unequivocal name *nomina nuda* in my petition.

DOCUMENT 33/15

By JOHN BALFOUR-BROWNE, M.A.

(British Museum (Natural History), Department of Entomology, London)

Letter dated 25th March, 1952

GENERIC NAMES PUBLISHED IN SYNONYMIES (REF. Z.N.(S.)387)

I have noticed your request in volume 7 of the *Bulletin of Zoological Nomenclature* for views on this matter and accordingly send you the following :—

- (a) It is not the practice (in Coleoptera), to accept as nomenclatorially available a manuscript generic name or a generic name which had previously only existed as a published *nomen nudum*.

I am strongly in favour of a reversal of the Paris decision, *i.e.*, I consider that it should be made clear in the *Règles* that a generic name published in this manner does *not* acquire any standing in Zoological Nomenclature. My view would not be altered by any circumstance ; I have always regretted the Paris decision.

Question (b) does not arise, as I answer (a) in the negative.

DOCUMENT 33/16

By G. H. E. HOPKINS, O.B.E., M.A.

(British Museum (Natural History), Zoological Museum, Tring, Herts, England)

Letter dated 30th April, 1952

GENERIC NAMES PUBLISHED IN SYNONYMIES

(COMMISSION'S REFERENCE Z.N.(S.)387)

In *Bull. zool. Nomencl.* 7 : 115 you ask for replies from specialists to three questions about the availability of manuscript generic names, or generic names previously published as *nomina nuda*, when published as synonyms. In the groups on which I work (Mallophaga and Siphonaptera) I cannot recall any instances of generic names coming within this category (unless an instance in which a manuscript generic name was published as a synonym of *several* valid generic names, in the same book is an example of it), but (as you remark on p. 114) the problem of trivial names similarly published in synonymies is identical in principle. My replies to the questions are, therefore, based solely on the position with regard to trivial names, but I do not at present see any reason for treating the two classes of names differently.

(1) In the groups on which I work (Mallophaga and Siphonaptera) it has never been the practice to regard publication in synonymy of manuscript names or names previously published as *nomina nuda* as conveying on them any status of availability. In our *Check List of the Genera and Species of Mallophaga*, just published, Miss Clay and I attempted to apply the decisions of the Paris Congress, and therefore (very reluctantly) treated all such names as available, but fortunately we found no instances in which this action necessitated acceptance of a name that had been published in synonymy after being a *nomen nudum* or a manuscript name.

(2) I am strongly opposed to such names being accepted as having been validly published.

(3) I am in favour of a reversal of the Paris decision, but as availability of such names would not have any important effect so far as the groups with which I deal are concerned, I would be willing to reconsider this attitude if it should be found that in other groups such a reversal would cause an appreciable amount of name-changing.

DOCUMENT 33/17

By **Th. HALTENORTH** (*Museum, München, Germany*)

Statement, dated 14th May, 1952, communicated by Professor E. M. Hering

IV. **B.1** sollte massgebend sein.

Bezüglich "C" sollte erst der Begriff des Ms. genau festgelegt werden, da es heute viele Vervielfältigungsverfahren gibt, die zwischen Ms und Buchdruck stehen.

Explanatory Note

The foregoing comment was prepared by Dr. Haltenorth in response to an invitation by Professor E. M. Hering who had prepared a synopsis of the problems at issue for consideration by German zoologists. It is to this document, the relevant extract from which is given below, that the number cited by Dr. Haltenorth refer.

.

B. Für die Entscheidung gibt es 3 Möglichkeiten :

1. Man sieht den Namen als Bezeichnung eines *Artengemisches* an, und die Entscheidung trifft nach den "Regeln" der *erste auswählende Autor*.

2. Man könnte annehmen, dass der *gleiche Name zweimal für verschiedene Arten* gegeben sei, und der zweite Name sei ein Homonym zum ersten, müsste also auf Grund der in Paris 1948 festgelegten *Seiten und Zeilenpriorität als Homonym* vorwerfen werden. Da in der betreffenden Veröffentlichung die Entscheidung darüber nicht immer möglich ist, müsste in gewissen Fällen Entscheidung der INK. angerufen werden können.

3. Der Name könnte, *wenn er ausdrücklich als Ersatzname* angegeben wurde, *nur auf den zu ersetzenden* angewendet werden, wenn auch irrtümlich eine andere Art als identisch mit diesem bestimmt wurde. Es müsste also der Ersatzname ausdrücklich mit dem Zusatz *nom. nov. bzw. nom. mut. bezeichnet* werden.

DOCUMENT 33/18

By THE COMMITTEE ON NOMENCLATURE
OF THE CHICAGO NATURAL HISTORY MUSEUM

Extract from a letter dated 15th May, 1952

The Committee on Nomenclature of the Chicago Natural History Museum proposes modifications of and additions to the Rules of Zoological Nomenclature as below stated. These proposals* are submitted to the International Commission more than one year in advance of the International Zoological Congress at Copenhagen with the request that they be put to a vote at the Section of Nomenclature of said Congress.

Proposal 2

We propose that the citing of a *nomen nudum* in synonymy is not to be considered as a valid *indication* in the sense of Article 25.

Reasons : We subscribe wholeheartedly to the reasons stated by Mayr in his petition to the Commission.ϕ

Karl P. Schmidt

Dr. Fritz Haas

Loren P. Woods

Robert F. Inger

A. L. Rand (mostly reservations about the form of Proposal 3)

Emmet R. Blake

Rupert L. Wenzel

Colin Campbell Sanborn

Clifford H. Pope

D. Dwight Davis

* The other proposals submitted in this letter are being published at the appropriate points in the present volume.

ϕ See Document 33/8.

DOCUMENT 33/19

By **J. R. DYMOND**

(University of Toronto, Department of Zoology, Toronto, Canada)

Statement received on 27th May, 1952

I am not in favour of the proposal that any status should be granted to manuscript names published in synonymies.

DOCUMENT 33/20

By **ERICH M. HERING**

(Abteilungsleiter am Zoologischen Museum der Humboldt-Universität zu Berlin)

Received on 7th June, 1952

STELLUNGNAHME ZU DEN VOSCHLÄGEN ÜBER DEN NOMENKLATORISCHEN STATUS VON NAMEN, DIE IN SYNONYMIEN-LISTEN VERÖFFENTLICHT WURDEN

Zu den im *Bull. zool. Nomencl.* 7: pt. 4, pp. 109-118 ausgeführten Vorschlägen nimmt der Stab der Zoologen am Zoologischen Museum in Berlin in folgender Weise Stellung :

(1) Die Gültigkeit von Genus-und Speziesnamen in Synonymien-Listen wird mit 13 von 14 Stimmen *abgelehnt*.

(2) Bei Anerkennung solcher Namen soll typische Art des synonymen Genus die Art sein, die auch typische Art des Genus ist, zu der das Manuskript-Genus als Synonym gestellt wurde.

(3) Die Einführung von Sicherungsklauseln wird in jedem Falle unterstützt.

Annexe to statement furnished by Professor Hering**Views of members of the Scientific Staff of the Zoologische Sammlung
des Bayerischen Staates, München**

Herr Dr. Walter Forster von der Zoologischen Sammlung des Bayerischen Staates, München, erklärt im Einvernehmen mit 11 von 12 befragten Mitgliedern des Zoologen-Stabes des Museums München, dass die Vorschläge unterstützt werden, Manuskriptnamen in Synonymien-Listen sollen Gültigkeit erhalten. Zu Punkt 2 und 3 entscheidet der Stab des Museums München in derselben Weise wie der des Museums Berlin.

DOCUMENT 33/21

By **JOSHUA L. BAILY, Jr.** (*San Diego, California, U.S.A.*)

Statement received on 24th June, 1952

(REFERENCE : Z.N.(S.)372)

A *nomen nudum* has no standing whatever, since it has no discernible meaning. If it is subsequently published in a synonymy it acquires a meaning and ceases to be a *nomen nudum* but under the present rules does not acquire legal standing. If subsequently it is accepted it acquires legal standing, but in such a case it should be credited to the writer who legalised it and it should date from its legalisation, not from its origination. Any later writer who wishes to use a double authority citation should be allowed to do so, but in such a case the use of the name of the legalising authority should be obligatory, and the use of the name of any other writer permissive only.

DOCUMENT 33/22

Statement of the views of the

**SCIENTIFIC STAFF OF THE ROYAL ONTARIO MUSEUM OF ZOOLOGY
AND PALAEONTOLOGY, TORONTO, CANADA**

Enclosure to a letter dated 26th June, 1952, from
Dr. F. A. URQUHART, Director

(For the text of the above letter, see Document 1/39)

**GENERIC NAMES PUBLISHED IN SYNONYMIES
(COMMISSION'S REFERENCE Z.N.(S.)387)**

Not a single member of our curatorial committee has ever been involved in the matter as presented by you in paragraph 10, page 115 of volume 7, part 4.

We are unanimously in favour of the suggestion that it should be made clear in the rules that a generic name published in the manner outlined by you does not thereby acquire any standing in zoological nomenclature.

DOCUMENT 33/23

By **A. MYRA KEEN** and **SIEMON W. MULLER**
(Stanford University, Stanford, California, U.S.A.)

Enclosure to a letter dated 1st July, 1952

**COMMENT ON THE QUESTION OF THE SPECIES TO BE ACCEPTED AS
THE TYPE SPECIES OF A NOMINAL GENUS, THE NAME OF WHICH WAS
PUBLISHED PRIOR TO 1st JANUARY, 1931, IN THE SYNONYMY OF A
GENUS**

In response to the query as to present practice in special fields of zoology or paleontology, we cite the well-known name *Thyasira* in Mollusca. This appears in Lamarck's "Histoire Naturelle des Animaux sans Vertèbres, vol. 5, 1818, p. 492, in the catalogue of *Amphidesma*. The last entry in the synonymy of *A. flexuosa* ("Lamarck") [= *Tellina flexuosa* Montagu, 1803] reads, "*Thyasira flexuosa*. Leach". Subsequent authors, noting Lamarck's statement that his specimens came from England, sent by Mr. Leach, have attributed the name to Leach or to Lamarck *ex* Leach MS.

This method of proposal is not common in Mollusca but has been accepted without question. We should approve the establishment of an appointed date, such as January, 1931, after which such introduction should be considered invalid.

DOCUMENT 33/24

Statement of the views of the **COMMITTEE ON NOMENCLATURE OF
THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK**

Enclosure to a letter dated 10th July, 1952, from

Dr. JOHN T. ZIMMER, Chairman

(For an extract from the above letter, see Document 1/41)

**GENERIC [AND SPECIFIC] NAMES FIRST PUBLISHED
IN THE SYNONYMY OF OTHER NAMES Z.N.(S.)387.**

The Commission has held that such names are available, but there is a question whether the provision should not be revised to exclude certain kinds of such names. Dr. Mayr has a petition* before the Commission to that effect and this committee supports at least a modified version of it (which modification Dr. Mayr has already suggested). This would exclude names depending solely on their inclusion in the synonymy for recognition. If accompanied by specific indications of identity, even if not accepted by the author, they would be available for the zoological entity concerned and the inclusion in synonymy of the accepted name would be considered as an error of identification. Quoted synonyms without individual identification would be counted as *nomina nuda* without prejudice to later proposal in acceptable manner. These provisions would affect only names published before 1st January, 1931; subsequent dates are covered by Article 25, (c).

If this proposal is rejected, the type species of otherwise unidentified generic names first proposed in synonymy are, *ipso facto*, identical with those of the name under which the manuscript names are synonymised. If a different type species is cited along with the name, it will stand as so designated since it serves to identify the supposed synonym as a distinct entity which may or may not prove to be identical with that covered by the accepted name.

A.M.N.H. Committee on Nomenclature.

(Signed) Edwin H. Colbert
John T. Nichols
Ernst Mayr
George H. T. Tate
John T. Zimmer (Chairman)

* See Document 33/8.

DOCUMENT 33/25

By **C. W. WRIGHT** (*London*)

Enclosure to a letter dated 18th July, 1952

GENERIC NAMES IN SYNONYMIES (Z.N.(S.)387)**Para. 10.**

Question (1). I work in Cephalopods (Ammonoidea). I only know of one generic name first published in a synonymy. That name has been mentioned once subsequently to my knowledge, and as if a valid name.

Question (2). I am emphatically not in favour of the Paris Congress decision.

Question (3). I am in favour of reversing the Paris Congress decision even if it does cause some instability to do so. Individual cases where a generic name was first published in a synonymy which specialists desire to retain should be dealt with by application to the Commission for exercise of plenary powers.

DOCUMENT 33/26

By J. CHESTER BRADLEY (*Cornell University, Ithaca, N.Y., U.S.A.*)

Statement dated 18th July, 1952

NAMES PUBLISHED IN SYNONYMY

In answer to the questions asked in paragraph 10 (page 115) of the Secretary's paper in vol. 7 of the *Bulletin of Zoological Nomenclature*. (1) In Hymenoptera and Coleoptera it has not been usual, so far as I am aware, to recognise a manuscript name as having been validated by reason of being published in synonymy of a previously published name. I have no doubt but that in some instances such names have been recognised as a result of *Opinion* 78. (3) I am in favour of a rule to the effect that publication of a manuscript name or republication of any other *nomen nudum* in synonymy does not in itself validate that name. I should not change my view if it were found that in other groups such a rule would cause name-changing. In either case I should provide that a name which had become established as a result of applying the principle enunciated by Secretary Stiles in discussion of the case involved in *Opinion* 78 should be referred to the Commission, and that the Commission should be instructed to decide each such case in the manner least disruptive of usage.

In attempting at Paris to codify the principles of *Opinions*, 4, 53 and 78, certain very important considerations brought out by Dr. Mayr were not perceived. This led us into a theoretically illogical and contradictory position which certainly must be rectified.

As pointed out by Dr. Mayr, citation in synonymy (except by demonstrating that the types are one and the same specimen (or species in case of a genus) is *always subjective, always subject to reversal*, and very frequently (from a zoological viewpoint) wholly erroneous.

No author who cites a manuscript name in synonymy has an intention of erecting a new species or genus, objectively synonymous with the one in the synonymy of which he lists it, he has no thought that its types shall be those of the latter instead of the specimens which circulate or exist in some museum under the manuscript name. To force such a conclusion is an absurdity. Hoyle, in dissenting from *Opinion* 53, was perfectly correct: "Kaup never proposed or suggested the formation of a species *Halicampus grayi**. The meaning of his bibliographical reference is that a specimen . . . in the British Museum which had . . . been labelled by him '*Halicampus grayi*' was really,

* He listed the manuscript name *Halicampus grayi* in synonymy of *Halicampus conspicillatus*.

in his opinion, referable to *Syngnathus conspicillatus* Jenyns. Whether he was right or wrong in this opinion does not seem to me to affect the question at issue. He did not create a new species”.

It should be borne in mind that what is ordinarily referred to as “publishing the synonymy of a species” consists first of an objective bibliographical reference to the original description, followed by a subjective listing of names that in the opinion of the author are *in varying degree* synonymous. One cannot tell at all which of the names he considers to apply to a form that differs in no respect from the type, or which apply to variants, forms, geographical subspecies, etc., all of which in his view constitute a single species. How then could one even infer the degree of synonymy of the specimens that are represented by the manuscript name that he lists? I have put this question at the specific level, but the same reasoning applies to the genus.

I therefore propose that it be agreed to recommend to the next Zoological Congress:

- (1) That in conclusion 18, *Bull. zool. Nomencl.* 4: 145, the phrase at bottom of p. 145 and top of p. 146 “is published in conditions which satisfy the requirements of the proviso to Article 25” shall be so modified so as to make it clear that the description or other validating items must be specifically directed to the previously manuscript name or *nomen nudum* which is thereupon being validated, and not merely indirectly by reason of being applied to another name to the synonymy of which the previously manuscript name is being relegated.
- (2) That in conclusion 47, *Bull. zool. Nomencl.* 4: 563, the matter in parentheses (round brackets) forming lines 9, 10 and part of 11 of the last paragraph be stricken out.
- (3) That words be inserted in Article 25 expressly stating that in no case shall the mere listing of a manuscript name or of a *nomen nudum* in the synonymy of a genus or of a species confer any status of validity or availability upon the name in question, nor shall it confer upon it any status under the law of homonymy.
- (4) That, in view of the prior *Opinion* 78, now cancelled, that a *Recommendation* should be inserted in Article 25 requesting any taxonomist who finds a name in current use by reason of its having been supposed that its citation in synonymy of another name conferred nomenclatorial validity upon it to bring the case to the attention of the International Commission on Zoological Nomenclature.
- (5) That words be inserted in Article 25 directing the Commission, in any such case to act if necessary under its plenary powers, in order to conserve the most customary usage.

DOCUMENT 33/27

By **HENNING LEMCHE** (*Universitetets Zoologiska Museum, Copenhagen*)

Letter dated 20th July, 1952

Concerning nominal genera published in synonymies, I think that we made an error in Paris. When at the time the whole problem was under discussion, those present had their attention so directly drawn towards the question concerning the type species that they—so to say by a slip—did not realise that the other question, that of the validity of the genus itself, was more than problematic. I am quite sure, at least, that this happened to me. It is probably the reason why no discussion at all arose on that point.

As the new *Règles* have not been published definitely till now, and as it is very difficult to pick out of the bulky Minutes of the Paris Meetings, I think that very few people will have made any corrections in nomenclature according to the decision in question made in Paris, and so, we probably are to reverse our decision immediately in order to prevent the confusion that may else arise.

In my own group, the *Opisthobranchs*, I do feel that so few authors really know about the details of the rules of nomenclature that there is no established "practice" in this minor question. However, I feel certain that much less difficulty is involved if we keep to clear and easily handled rules. It is by far the more easy to say quite generally (as has been done hitherto): If a name has not been proposed for *use*, it is no real name. Then, we escape all the difficulties you have been asked to study in detail.

As the plenary powers are available in *all* cases where it is advisable, there should be no special reference thereto.

DOCUMENT 33/28

From **E. RAYMOND HALL**

(*University of Kansas, Department of Zoology, Lawrence, Kansas, U.S.A.*)

(Enclosure to a letter dated 22nd July, 1952)

Rule as unavailable if unaccompanied by a description.

DOCUMENT 33/29**Views of certain members of the NOMENCLATURE COMMITTEE OF THE
SOCIETY OF SYSTEMATIC ZOOLOGY**

Letter, dated 25th July, 1952, with enclosures, from
Mr. W. I. FOLLETT, Chairman

GENERIC NAMES FIRST PUBLISHED IN SYNONYMY : Z.N.(S.)387

I enclose herewith copies of comments on this subject, received from members of the Nomenclature Committee of the Society of Systematic Zoology.

Dr. Ellsworth C. Dougherty, who is also a member of this committee, submitted a paper entitled "The Species to Be Accepted as the Type Species of a Nominal Genus, the name of which was first published in a Generic Synonymy," at the recent symposium conducted by the Pacific Section of the society. Unfortunately I have not available a copy of Dr. Dougherty's paper, but I am informed that he has furnished you with a copy, for publication in the *Bulletin of Zoological Nomenclature*.*

Enclosure 1 to Mr. Follett's letter of 25th July, 1952

Extract from a letter, dated 20th May, 1952, from **HENRY TOWNES**
(*North Carolina State College of Agriculture and Engineering of the
University of North Carolina, Raleigh, North Carolina, U.S.A.*)

(4) Names published only in synonymy were never "proposed" and therefore are not available.

* See explanatory note in Document 33/33.

Enclosure 2 to Mr. Follett's letter of 25th July, 1952

Note prepared by Mr. Follett setting out views expressed to him orally by
Professor **ROBERT L. USINGER** (*University of California, Berkeley,
California, U.S.A.*) at Berkeley on 25th June, 1952

(Dr. Usinger is absent in the field, and in order to meet Mr. Hemmings's deadline I am submitting this statement on Dr. Usinger's behalf, and am sending him a carbon copy so that he may supplement it as may seem desirable to him on his return.—W. I. F.)

I am inclined to agree with Dougherty. The two critical points are :
(1) a generic name published in a generic synonymy is automatically provided with a definition in accordance with Article 25, and should be available ; and
(2) the question has to be decided one way or the other.

Enclosure 3 to Mr. Follett's letter of 25th July, 1952

Statement prepared by **Dr. JOHN T. ZIMMER**
(*The American Museum of Natural History, New York*)

387. I favour the exclusion of names, generic and specific, first published in the synonymy of other names without specific identification of their own. Thus, an author may give characters (or references to such) but still refuse to adopt such names, in which case the name would be available. If "identifiable" solely by inclusion in synonymy, it should be considered a *nomen nudum*. This, of course, covers only names published prior to 1931.

If this proposal is rejected, the type species of the synonyms in question must be identical with that of the name under which they are synonymised. If a different type species is cited at the time of this original publication, it identifies the name and removes it from obligatory synonymy, coming under my previous paragraph.

DOCUMENT 33/30

Statement of the views of certain members and former members of the

**COMMITTEE ON ZOOLOGICAL NOMENCLATURE OF THE
AMERICAN SOCIETY OF ICHTHYOLOGISTS AND HERPETOLOGISTS**

Letter, dated 26th July, 1952, with enclosures, from

Mr. W. I. FOLLETT, Chairman

GENERIC NAMES FIRST PUBLISHED IN SYNONYMY: Z.N.(S) 387

The enclosed copies of correspondence reflect the views of members (and former members) of the Committee on Zoological Nomenclature of the American Society of Ichthyologists and Herpetologists, concerning the problem herein above mentioned.

Enclosure 1 to Mr. Follett's letter of 26th July, 1952

Extract from a letter, dated 13th April, 1952, from **CHARLES M. BOGERT**
(*The American Museum of Natural History, New York*)

Second, with reference to the problem of *nomina nuda* . . . It seems sufficiently plain that Stiles' opinion was so completely outlandish and was so manifestly misguided that it should obviously be corrected. However, it would do no harm to have the Committee undertake to express this opinion to the International Commission, taking for granted that it represents the consensus of the Society without a poll. I can see no necessity for revising the enclosed memorandum*, as already modified and improved by Ernst Mayr.

Enclosure 2 to Mr. Follett's letter of 26th July, 1952

Extract from a letter, dated 8th May, 1952, from **ARNOLD B. GROBMAN**
(*University of Florida, Gainesville, Florida, U.S.A.*)

I am in substantial agreement with Mayr and Myers regarding the necessity for the International Commission to consider the problem of *nomina nuda* and would be pleased to have Mayr's memorandum considered an expression of my views.

* The document by Dr. Ernst Mayr referred to in this and following enclosures to Mr. Follett's letter is assumed to be that submitted to the International Commission and now reproduced as Document 33/8.

Enclosure 3 to Mr. Follett's letter of 26th July, 1952

Extract from a letter dated 23rd April, 1952

from **ROBERT RUSH MILLER**, (*University of Michigan, Ann Arbor, Michigan, U.S.A.*)

Regarding Ernst Mayr's memorandum "The Status of *nomina nuda* Listed in Synonymy" I have the following comments to offer :

I favor the conclusions reached in this memorandum but I believe that the Commission has already acted in like manner (see *Bull. zool. Nomencl.*, vol. 4, pp. 148-149 and 345 ; 1950).

...In the event that I have read the "new Rules" too hastily (hence incorrectly), and action is called for, I would favor support of Dr. Mayr's memorandum in principle as an expression of the consensus of the A.S.I.H.

Enclosure 4 to Mr. Follett's letter of 26th July, 1952

Extract from a letter, dated 16th April, 1951, from **HOBART M. SMITH** (*University of Illinois, Department of Zoology, Urbana, Illinois, U.S.A.*)

Material relating to the status of *nomina nuda* listed in synonymy have been received and digested. My opinions on the points listed follow.

... As for my personal opinion : some of Mayr's points are good, others not. Some extensively exploited approaches are, so it appears to me, strongly biased and not wholly logical. Especially unacceptable are paragraphs 3 and 4 of page 2. Bells would indeed toll for the stability of taxonomy if any recognition whatsoever were to be given the specimens upon which *nomina nuda* are based because of their role as onomatophores of *nomina nuda*. It would be highly dangerous to give taxonomic recognition as an onomatophore to any specimen not adequately described as well as named.

The discussion does not adequately emphasise the difference between authoring a name and authoring a species. Existing terms make it difficult to avoid ambiguity on my own part. But to proceed : in the normal course of describing a species, a student does two things (with which I am here concerned): he proposes a name, and he describes the (we hope) single onomatophore which bears that name. For recognition of the species to which that name and onomatophore belong, which is more important—the name itself, or the onomatophore (or description thereof, actually, since it, not the specimen itself,

is published)? Clearly the description is the meat, the name but a shell. One can recognise a species by description, not by a name alone. It follows that the author of the description is the "author of the species" or of the scientific name, and if he applies a name no one else has used for a description of a species it should stick. *The author of the words making up the name* is reasonably and actually immaterial. How often have we named something following a suggestion of a colleague? I have, plenty of times. Some authors have even stated that the name applied was given by someone else. What difference? The guy who applies it to a description (or indication, etc., etc.) still is, in my opinion, the author of the scientific name of the zoological entity involved.

I do not want to expand this view here. Let someone who knows his way around better than I handle it. I do hope to have made clear, however, that something can be said on the other side. Certainly recognition of *nomina nuda* cited in synonymy introduces problems; I think the problems are far more serious if one does not so recognise them, or at least if one does not adhere to the *principles* back of such recognition. Presumably an acceptable compromise could and should be reached. In any case use, perhaps generous use, of the plenary powers of the Commission probably will be needed.

DOCUMENT 33/31

By **W. E. CHINA**, M.A., D.Sc.

(*British Museum (Natural History), Department of Entomology, London*)

Extract from a letter dated 20th August, 1952

GENERIC NAMES PUBLISHED IN SYNONYMY (REF. Z.N.(S.)387)

My answer to Paragraph 10 on page 115 of your paper in vol. 7 of the *Bull. zool. Nomencl.* is "No".

I agree with Mayr that such a nominal genus is a *nomen nudum*. So far, I have not come across a similar case in the Hemiptera.

DOCUMENT 33/32

By **K. H. L. KEY** (*Commonwealth Scientific and Industrial Research Organisation,
Division of Entomology, Canberra, Australia*)

Extract from a letter dated 4th September, 1952

(Note : The main portion of the above letter was concerned with the problem of the emendation of names, on which a separate paper was submitted by Dr. Key and which constitutes Document 5/26 of the Copenhagen Series.)

I have studied your proposals in connection with the other six questions referred to you by the Thirteenth Congress, and in general I find myself in hearty agreement with them.

DOCUMENT 33/33

By **ELLSWORTH C. DOUGHERTY**, Ph.D., M.D.

(*Department of Zoology, University of California, Berkeley, California, U.S.A.*)

Editorial Note : Attention is drawn to a paper entitled "Problems of Nomenclatorial Practice now under consideration by the International Commission : Secretary Francis Hemming's Fourth and Fifth Problems" which has been included in the Copenhagen Series, as Document 6/19 of the present volume. In the foregoing paper Dr. Dougherty concludes as follows : "In conclusion, therefore, it is my feeling that a simple and desirable solution of the problem of determining type species for nominal genera first published in generic synonymies would be to construe them as falling under the provisions of Article 30, Rule (f), as clarified and restricted by Secretary Hemming and by me". (Int'l'd) F. H.

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Triple-Part 3/5 (Emendation of names)

It is regretted that technical reproduction difficulties are still holding up the publication of this Triple-Part. Publication is expected shortly after the present Part.